Preschool and Parental Response in a Second Best World: Evidence from a School Construction Experiment

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Abstract

Interventions targeting early childhood development hold promise for increasing human capital and reducing the intergenerational transmission of poverty. This paper presents results from a randomized evaluation of a preschool construction program in Cambodia, and suggests caution. The overall impact of the program on early childhood outcomes was small and statistically insignificant. For the cohort with highest program exposure, the impact on cognitive indicators was negative; with the largest negative effects among children of poorer and less educated parents. The results are consistent with frequent underage enrollment in primary school in the absence of preschools, stricter enforcement of the minimum age for primary school entry after the intervention, substitution between primary and preschool following intervention, and difference in demand responses to the new preschools between more and less educated parents. The results show that contextual and program specifics, and behavioral responses, can potentially lead to perverse effects of programs.

1. INTRODUCTION

At least 200 million children in the developing world fail to achieve their potential in terms of cognitive and overall development (Grantham-McGregor, et al. 2007). Cognitive development in early childhood is important in its own right and, in addition, low levels of cognitive development are often associated with inadequate school readiness which possibly contributes to poor school performance. Low levels of cognitive development, amplified with poor school performance can undermine children's potential future economic success (J. Heckman 2008). Studies from both developed and developing countries that track individuals from early childhood into adulthood show that children brought up in a more favorable early environment are healthier and taller, have higher cognitive ability and educational attainment, and earn significantly higher wages (Paxson and Schady 2010, Stith, Gorman and Choudhury 2003, Liddell C 2001, Walker, et al. 2005, Gertler, et al. 2014, Havnes and Mogstad 2011). Neuro-scientific evidence confirms that early childhood is a critical step in human development: it is the period in which the development of the synapse, the connections between neurons and the child's ability to absorb new sounds and languages occurs (Shonkoff and Philips 2000). Consequently, policymakers in many countries are increasingly seeing early childhood as a particularly promising period to target when trying to address socio-economic gaps in human capital development.

Little is known about parental responses to the introduction of new early childhood programs in developing countries. Whether and how the availability of new programs, such as preschools, translates into better cognitive and socio-emotional outcomes for young children will depend on parental behavior responses. Parents may be unwilling to leave the child with an unknown adult at early ages, or not appreciate the value of preschool for early childhood development, or believe that socio-emotional development is better carried out in the home.[2] If such beliefs are positively correlated with lower levels and quality of parental investment, children who need pre-school the most may not be the ones that are sent to preschool (Blau & Currie, 2006).

This paper studies parental response to, and subsequent impact on cognitive outcomes of, a preschool construction program in Cambodia. We rely on an experimental design to evaluate

[2] See for instance the potential effects of "redshirting" (delayed entry into formal schooling) in developed countries (Black, Devereux, & Salvanes, 2011)

the impacts of a relatively large-scale intervention. The program, implemented by the government of Cambodia (partly funded by a grant from the Education Fast Track Initiative administered by the World Bank), involved the construction of preschool classrooms within the primary schools of poor rural villages. This was accompanied by training, deployment, and supervision of new preschool teachers and the provision of materials. The new preschools were integrated within the regular Cambodian public educational system. We therefore study the short-term impacts of increased access to government preschools on both cognitive and non-cognitive outcomes of 4 to 6 year old children. Importantly, in Cambodia, the counterfactual to preschool was not only parental care at home, but also informal underage enrollment in primary school. This paper hence estimates the effect of a pre-school program in a context where the intervention might trigger reallocation between preschool, primary school and parental care at home.

The results suggest limited overall impacts on child development. Poor implementation led to limited exposure time and to poor service quality. Parental response led to substitution between underage enrollment in primary school and preschool enrollment for some children, and for others it led to withdrawal from any formal education. We find negative impacts on cognitive development for five year olds, and these negative effects were the largest for children from less educated and poorer parents. These results indicate that the design of interventions such as this should start with a good understanding of parental and teacher decision-making. More generally, they show how implementation and behavioral responses might not only limit positive impacts, but could even lead to perverse impacts of interventions.

The rest of the paper is organized as follows: Section 2 further reviews the literature; section 3 describes the intervention, the experimental design and the data; section 4 discusses implementation and take-up of the program; section 5 presents the empirical strategy and the main impact evaluation results, including robustness checks; Section 6 interprets and discusses the findings; Section 7 concludes.

2. LITERATURE REVIEW

In the United States, most of the observable cognitive gap between wealthier and poorer children is already present before children enter school, and early cognitive and non-cognitive traits are strong predictors of success in term of subsequent school attainment, economic status

(Chetty, et al. 2011), criminality (Currie 2001) and social behavior (Heckman, Stixrud and Urzua 2006). Similarly, in many developing countries, there are steep socioeconomic gradients in early childhood cognitive development—children from poorer households show significantly worse outcomes that often grow with age (Halpern, et al. 1996, Ghuman, et al. 2005, Grantham-McGregor, et al. 2007, Fernald, et al. 2011, Naudeau, et al. 2011, Schady, et al. 2014). There are increasing findings that early stimulation, even after an initial period of fade-out, may trigger large long-term effects. Various early stimulation interventions such as a large class-size reduction programs (Chetty, et al. 2011), a formalized preschool intervention (Schweinhart, et al., 2005) or an early social skill training (Algan, Beasley, Tremblay, & Vitaro, 2014) establish long-term effects of early childhood interventions. Such findings motivate dynamic skill formation models (Cunha, Heckman, & M. Schennach, 2010) that also point to early investment as the most effective in reducing gaps in cognitive attainment. Based on these findings, policies targeting early childhood development are often believed to be cost-effective and inequality-reducing interventions. Promoting cognitive and overall development among disadvantaged children from early on is expected to provide a better base for learning in primary school and in later stages of life and, as such, to help break the intergenerational transmission of poverty.

Preschool interventions for children in the 3 to 5 age group are often assumed to hold considerable promise to achieve those goals. Compared to parental care at home, preschools are thought to better prepare children for a more structured primary school environment, and interactions with professional teachers and with peers are often thought to increase both cognitive and non-cognitive outcomes—especially for disadvantaged children whose low-educated parents might not be able to provide similar stimulation at home. Duncan and Magnuson (2013) review the evidence for the US and conclude that impacts are mostly in line with these expectations. They also note, however, that the results from programs implemented for large and representative populations are generally much smaller than those found for small-scale pilot programs.[3] Evidence regarding preschool interventions in developing countries is generally also positive, though mostly based on non-experimental designs. In Uruguay, the expansion of the provision of preschool education led to significant and positive effect of pre-primary education on school attainment via a reduction in drop-outs (Berlinski, Galiani and

[3] Some of the best known experimental evidence such as Abecedarian, Perry Preschool, or the "Milwaukee Project" that show large impacts (Currie 2001) were pilot programs with small number of participants (Perry preschool impacts were evaluated on 123 observations, abecedarian on 111 observations).

Manacorda 2008). In Argentina, Berlinski, Galiani and Gertler (2009) found significant effects on school competencies three years after children participated in a pre-primary school class. Behrman, Cheng and Todd (2004) show cognitive and psychosocial effects of a preschool program (with health and psychological components) on children aged between 6 to 59 months in Bolivia, with no effects found before seven months of treatment exposure. Non-experimental evidence from Cambodia suggests positive impacts of preschool programs on child development (Rao et. al. 2012)—a result that contrasts with the findings we present here (we return to the reasons for this difference in section 5). To the best of our knowledge, Martinez, Naudeau, and Pereira (2012) is the only large-scale experimental evaluation of preschool versus no preschool availability in a low-income country. They show positive impacts on the cognition, subsequent school participation, and socio-emotional development of children participating in an NGO-implemented program combining preschool and a parenting intervention in rural Mozambique.

Evidence from other types of early childhood programs suggests that there are potentially large impacts of improving parental investments early in life. Most notably, evidence from a randomized control trial in Jamaica points to strong and lasting impacts of early childhood stimulation, both in the short-term (Grantham-McGregor, et al. 1991) as well as the long-term—impacts of the early psychosocial stimulation were still detectable on risky behaviors, criminality, IQ tests and labor market outcomes when recipients reached 22 years old (Grantham-McGregor, et al. 2007, Gertler, et al. 2014). Positive results on cognitive and non-cognitive outcomes were also found for similar stimulation programs aimed at changing parental caregiving practices at home in Colombia (Attanasio et al, (2012), Nicaragua (Macours, et al. 2012), Bangladesh (Nahar, et al. 2009, Aboud and Akhter 2011) India (Bentley, et al. 2010), Chile (Lozoff, et al. 2010), and Pakistan (Gowani, et al. 2014). Most of these interventions were implemented at small scale and combined early stimulation with nutrition-specific interventions. While the stimulation intervention consistently benefited child development, little evidence was found of synergistic interaction between nutrition and stimulation on child development outcomes (Grantham-McGregor, et al. 2014). Finally cash transfer programs—often large scale and with conditionalities targeting parental decisions on health practices and nutrition—have led to significant improvements in health outcomes in Mexico (P. Gertler 2004) and cognitive development in Ecuador (Paxson and Schady 2010) and Nicaragua (Macours, Schady and Vakis (2012); Barham, Macours and Maluccio (2013)).

In sum, the existing evidence confirms the "proof of concept" that early childhood

interventions, including preschools, can have positive impacts. However, much of this evidence comes from small scale programs with committed implementing partners and with non-representative samples, which potentially affects their external validity. Recent evidence from other (non-ECD) education programs shows that impacts during scale-up by government agencies can be quite different from those obtained in smaller scale NGO projects (Bold, et al. 2013). So far, there is very little evidence regarding larger-scale educational system interventions specifically targeting early childhood cognitive development and school readiness from developing countries.

3. THE PRESCHOOL PROGRAM, EXPERIMENTAL DESIGN AND DATA

Cambodia's Ministry of Education Youth and Sports started a large-scale effort to scale-up preschool availability in 2009. The goal was to increase access to preschools through the construction of a preschool classroom within newly renovated primary schools in 138 villages situated in disadvantaged rural areas. Before the intervention, preschool attendance at the national level was only 12% (Rao and Pearson 2007). The scale of the intervention and the fact that it was implemented by the government (as opposed to a dedicated NGO) makes this an interesting setting for an evaluation with potentially high external validity.

The newly built preschool classroom was open to children between 3 and 5 years old. In practice, 5 year olds were prioritized for enrollment, reflecting the program's goal to increase subsequent enrollment in primary school and children's adjustment to the formal school system (the official age for entry into the first grade of primary school is 6 years old). In addition to construction and the provision of teaching materials (books, tables, etc…) the program included provision for preschool teacher recruitment, training, salaries and supervision. Parents were expected to be responsible for the purchase of additional learning materials such as a pen, a pencil, chalk, a slate board and a notebook.[4] The preschool curriculum was composed of singing, drawing (mixing colors, reproducing signs/geographic figures on a board or with small sticks), physical activities (such as gymnastic and games), some vocabulary (listing words), and counting. It was designed for the 3 to 5 age group and did not explicitly include writing or reading. Teachers often organized social games in which children had to recall the name of other students, and add or subtract them from a group of pupils.

[4] Children are expected to wear a uniform but this rule is rarely enforced.

The preschool program was integrated into a primary school rehabilitation effort and, therefore, targeted villages with a primary school that needed upgrading at the start of the intervention. This upgrading typically involved building additional classrooms so that the school would be able to cover from preschool through to grade 6 of primary school; sometimes it involved construction of an entirely new school building. The fact that the new preschool classrooms were established in conjunction with other construction may have had potential effects on primary school outcomes (for example through class size effects or through access to schooling for older siblings). As these effects are potentially relevant for the oldest cohort in our sample we return to this in point in the interpretation of the findings.

Among villages eligible for preschool construction, 26 villages were randomly selected to receive a preschool in the first school year of implementation (2009/10), while 19 villages were randomly selected as control.[5] The 45 villages were selected in three large provinces, making it unlikely that the treatment had any impact on children in the control villages. Baseline data were collected between December 2008 and February 2009 on a sample of children, ranging from 24 to 59 months old, sampled for their eligibility for preschool exposure during the planned program implementation period (Figure 1). Follow-up data on the same sample were collected between June and August 2011. The quantitative data were complemented by qualitative data collected after program implementation (May 2012) to increase understanding of the preliminary results of the evaluation, focusing on issues of program implementation, potential reasons for low program take-up, as well as the content of the intervention itself.

At baseline, up to 40 households with at least one child aged between 24 and 59 months old were sampled in each village.[6] In total, 1399 households, and 1731 children, were surveyed. Information about the household and the children was collected from caregivers, and a series of child development instruments was administered to all children between 36 and 59 months at baseline (these instruments were not adapted to younger children ages 24-35 months at baseline and therefore not administered to them). First, an adapted version of the Ages and

[5] All treatment and control villages were selected from a list of "eligible" villages. In villages selected as control, school construction and rehabilitation would commence 2 years after it had been carried out in treatment villages.

[6] When more than 40 target households were present in the village, a random sample was drawn based on a complete list of households with children in the targeted age group obtained from the village leader. With the conventional power level (80%) and significance level (5%), and the intra-cluster correlation of 0.043 for the Woodcock-Johnson test at baseline, this would have given a MDE of .18 standard deviation with full compliance and using a set of control variables

Stages Questionnaire (ASQ)[7] was translated into Khmer and administered to all children (with four different age-specific versions of the instrument, i.e., one for every 6-month age interval) to capture child development outcomes across various domains, including fine motor, gross motor, cognitive development (communication and problem solving), and social competencies. Results from this instrument are based partly on responses given or behaviors demonstrated by the child (i.e. for questions where a specific child response or behavior could be elicited in the context of the household visit), partly by those given by the caregiver. In addition, a translated Khmer version of the TVIP (Test de Vocabulario en Imagenes Peabody) was administered. The TVIP is a version of the Peabody Picture Vocabulary Test (PPVT) adapted and normalized for populations in low-income settings. It measures early receptive vocabulary acquisition and is often considered a good indicator of early cognitive development.[8] Finally a translated version of the Woodcock Johnson (WJ) associative (short-term) memory test was also administered. In the follow-up, all instruments were re-administered to children, with the exception of the social competencies of the ASQ. Instead, the Strength and Difficulty test (SDQ), a test of a socio-emotional competencies based on parental response, was added at follow-up. The SDQ provides a measure of children's potential problematic behavior (emotional, hyperactivity, conduct) and of their pro-social skills. [9]

Both rounds of data also include one cognitive test for the caregivers, the Raven Progressive Matrices test, and a parental involvement score, based on parents' responses to eight questions regarding engagement in education and cognitive development of their child.[10] The household survey further includes questions regarding the households' economic situation, medical care,

[7]Ages & Stages Questionnaires® (ASQ), Second Edition: A Parent-Completed, Child-Monitoring System, by Diane Bricker and Jane Squires. Copyright © 1999 by Paul H. Brookes Publishing Co., Inc. www.agesandstages.com. Used with permission of the publisher.

[8] While the original version of the TVIP was standardized for low-income populations in Mexico and Puerto Rico, the version used in the Cambodia context was translated into Khmer and was piloted and validated prior to baseline data collection with the support of key informants. Only raw TVIP scores (interpreted as the number of words correctly recognized by a child until a test is suspended) are discussed in this paper instead of externally standardized TVIP scores, as the later would explicitly benchmark the scores of children in the sample to the score obtained by children obtained in the reference sample to norm the test in Mexico and Puerto Rico, which would necessarily not be appropriate.

[9] In addition, the ASQ was administered to the younger siblings (age 36 to 59 months) of the target children at follow up. For all children in primary school, the EDI (Early Development Instrument) was also collected through observations in each primary school. The latter is not analyzed in this paper as data cannot be merged with the survey data.

[10] Parents were asked how often they read a book to the child, tell a story, sing songs, talk to, play games with numbers, play games with words, play active games, and teach to become self-sufficient. Possible answers are often, sometimes or rarely. The average score is calculated by assigning a score of 1, 2 or 3 to those possible answers. Results are similar when using alternative aggregation methods.

education background, and parental behavior. Separately, data on schools and villages were collected through interviews with the school director and the village leader.

Column 2 of Table 1 reports baseline characteristics of the sample. The children in our study are substantially disadvantaged: 54% of the children are stunted (height-for-age less than 2 standard deviations below the WHO standard) and 17% are severely stunted (lower than 3 standard deviations). In Cambodia as a whole, 45% of children were stunted and 16% were severely stunted (DHS- Cambodia 2010). Sample children live in relatively large families (5.7 members on average) whose income mostly comes from subsistence farming (average revenue from paid work is small). Less than half of the caregivers are literate. At baseline, almost 9% of the targeted children were attending any formal school, with 2.3% in preschool and 6.4% in primary school.

Columns 3 and 4 of Table 1 report the test result of the balance between treatment and control villages. In the full sample only one of the baseline characteristics (out of 28) is statistically significantly different between treatment and control groups, consistent with pure chance and therefore confirming that randomization produced comparable groups. Nevertheless, to avoid any potential bias, this variable (the number of children below 6 in the household) is controlled for throughout the analysis.[11] A similar conclusion is reached when looking at balancing on the five year olds and on other age groups (not display here).

The attrition rate across survey rounds is modest, and is not significantly different between treatment and control groups (10.8% for treatment and 10.4% for controls).[12] We also test whether attrition may have distorted the initial balance across the experimental groups by estimating the following model:[13]

$$A_{i1} = \gamma T_i + \beta y_{i0} + \delta(y_{i0} * T_i) + \varepsilon_i \qquad (1)$$

where y_{i0} is a baseline indicator (e.g. a baseline test score, or a child or household characteristic), T_i the treatment assignment of the village of child *I*, A_{i1} is a dummy variable indicating whether the child i was missing at follow-up, and $y_{i0} * T_i$ is the interaction between attrition and treatment. The coefficient β gives the baseline characteristics' association with

[11] In the analysis below we break the sample into age cohorts. Baseline characteristics are also well balanced for each of the cohorts. For the "5-year old cohort", for instance, only 2 out of 28 variables are significantly different at the 10% level.

[12] In a regression predicting attrition on the basis of treatment status, the coefficient on treatment is -0.004 with a standard error 0.026. The corresponding estimate for the 5 year old cohort is -0.012, standard error with a standard error of 0.027.

[13] This approach is similar to Becketti, Gould, Lillard, & Welch, (1988).

attrition in the control group while δ captures the attrition bias (that is, attrition differential between treatment and control groups). As discussed below we are particularly concerned with the effects of the program on the "5 year-old" cohort which was most affected by the program, and therefore present this analysis for the sample as a whole as well as for just this subsample. Table 2 presents resulting coefficient estimates. In the full sample, only one coefficient (mother's height) seems to be affected by attrition, on the 5 year old sample, both mother's height and child height-for-age seem to suffer from attrition bias. All other estimates of interest do not appear to have been affected by attrition. In order to minimize this potential bias, we include the child's baseline height-for-age and mother's height in the set of controls.

3.1. PRESCHOOL CONSTRUCTION COMPLIANCE

Administrative records of school construction show that compliance with the experimental design was imperfect: school upgrading occurred in two out of the 19 control villages (Table 3). Moreover, while construction in treatment villages should have started in early 2009, the first preschools only opened in January 2010, and most of them were only open to students in October 2010—the beginning of the 2010/11 school year. By June 2011, seven preschools in the treatment arm of the study were not finished. Information gathered from school directors and village chiefs paint a similar picture: directors in 5 out of 26 treatment villages report not having a functioning preschool by June 2011.

As most schools opened for school entry in 2010/11, the follow-up data capture the impact of the program after only one school year (7 months; see Figure 1). The delays also meant that preschool exposure of the oldest cohort (48-59 months at baseline) was limited, since all children 70 months or older in October 2010 were expected to enroll in primary school for that school year.

The delays and, in some villages, the incomplete preschool construction have implications for the analysis of impacts on tests scores, as the limited exposure duration reduces the likelihood of measuring impacts (Behrman and King 2009). That said, they also point to an important first lesson of this study, namely the implementation difficulties that can hamper the effectiveness of a program at scale such as this one (at least over the relatively short period discussed in this paper).[15]

[15] In parallel to this RCT, the government also implemented two types of new informal preschools (home based program and community based preschool) in other regions of the country. A separate RCT was set up to evaluate their impact, but implementation issues were even larger than for the formal

3.2. PRESCHOOL AND PRIMARY SCHOOL ATTENDANCE

Table 4 presents several measures of preschool and primary school attendance at follow-up as reported by children's caregivers. Children in the treatment group are significantly more likely to have participated in preschool, with a treatment-control difference of 25 percentage points. Consistent with the priority given to 5 year olds, the impacts in preschool attendance are highest for this group (32 percentage points). The relatively small differences between treatment and control are largely driven by low program take-up and, to a lesser extent, the almost 11% of children in control villages enrolled in preschool. Some of this is because of the construction that took place in control villages (2% of children), and some of this is because children were reported as attending a preschool despite there not being one in the village (8% of children). Qualitative field work indicated that the attendance rate is not driven down by capacity constraints. There was no enforced maximum limit of children per preschool, and school directors never reported refusing a child because of capacity constraints. Take-up was also not driven by the availability of alternative early childhood programs.[17] There are no significant differences across treatment and control villages in participation in alternative early childhood programs, and overall participation in other programs is low (6% of children in treatment villages had attended community-based preschools; 15% had home-visits or community meetings).

The relative low take-up of the program in the treatment villages suggests that there are constraints to preschool attendance that the school construction program did not address. Low parental demand for preschool could be explained by a lack of information or disinterest in preschool education. Other constraints suggested by parents in follow-up qualitative interviews included liquidity constraints, low quality of preschool supply (inadequate premises or low teacher quality), exposure of small children to violence at school, or parental time constraints. While parents never reported that registration had been declined by the preschool, it is possible that informal signals by teachers might have discouraged parents from enrolling their children.

While the experimental design of this evaluation does not allow disentangling the importance of the various mechanisms, Table 5 shows the correlates of preschool attendance (in villages with a preschool) that are consistent with some of these reported constraints. The results

program, leading to even lower level of compliance and take up, and no results on early childhood outcomes (Bouguen, et al. 2013).

[17] As mentioned (footnote 13) Home-based program and Community-Based Preschool were being set up at the same time by the government, but were targeted to other villages.

(reported as odds-ratios based on logit estimations) suggest that higher socioeconomic status is associated with higher levels of preschool participation: household revenue (calculated as the average sum of salary earned in a week per household adult member), mother's literacy, mother's score on the Raven's test and scores on the parental involvement questions are significantly positively associated with attendance. Living in a dwelling with a thatch roof and the number of children per household are negatively associated with preschool attendance. Overall, these findings point to inequalities in preschool enrollment that are related to households' socio-economic background

The low exposure duration and low program take-up in treatment villages, along with non-compliance in program implementation, result in a very small difference in average exposure to preschool treatment between children in the treatment and control villages. Overall the difference in average exposure is about two months. This will be important for the interpretation of the program impacts on cognitive and non-cognitive outcomes.

4. EMPIRICAL SPECIFICATION AND RESULTS

4.1. EMPIRICAL SPECIFICATION

To analyze the effect of the treatment on children's school attendance and cognitive and non-cognitive development we estimate a basic reduced form empirical model:

$$Y_i = \beta_0 + \beta_1 T_i + \beta_2 \boldsymbol{X}_i + \epsilon_i \quad (2)$$

With T_i indicating whether the village child *i* lives in was assigned to treatment, and $\boldsymbol{X}$ is a vector of control variables—a full set of monthly baseline age dummies, a gender dummy and interactions, province fixed effects, and two additional baseline control variables (the number of children under 6 in the household and baseline height-for-age). The main focus of our analysis is β_1, the intent-to-treat estimate, as school construction as a whole is the prime objective of the study and also because we have reasons to believe that our sample includes some *defiers* (see section 5.2).

For impacts on cognitive and non-cognitive test scores, we estimate the impact on each test separately, as well as the average effect across all tests following Kling and Liebman (2004). First, all scores are standardized using the standard deviation of the control group.[18] Then, each score is regressed individually on the treatment variable using a Seemingly Unrelated

[18] When necessary SDQ are reversed so that a positive result can be interpreted as a "better" outcome.

Regression (SUR) model. Average impacts on all, or subsets, of the outcomes variables and standard errors of those averages (accounting for potential correlation between standard errors of individual estimates) can then be computed. We calculate the overall average, as well as subgroup averages by development domain: a "Motor development index" (gross motor and fine motor), the "Anthropometrics index" (height-for age and weight-for-age), the "Cognitive development index" (the TVIP, the Woodcock Johnson memory test and the cognitive competences of the ASQ, that is "problem solving and communication") and "Non cognitive development index" (combining the different sub-domains of the SDQ questionnaire).

Impacts are analyzed over the entire sample of children and also estimated separately for three separate age cohorts. We do so to reflect on the timing of the (delayed) intervention and the priority given to preschool enrollment of 5 year olds. The three cohorts are defined as follows: children who were at baseline between 24 and 35 months, between 36 months and 47 months, and between 48 months and 59 months. Since baseline data were collected between December 2008 and February 2009, children in the first cohort were between 46 and 57 months old at the beginning of the 2010/11 school year (the effective start date of the intervention). We will therefore refer to this group as the "four year-old" cohort. Children in the second cohort were between 58 and 69 months old at the start of the 2010/11 school year. We refer to them as the "five year-old" cohort. Finally, children in the third cohort were between 70 and 81 months at the start of the 2010/11 school year. We refer to them as the "six year-old" cohort.

We focus much of our analysis on the five year-old cohort: This is the cohort with the largest differential take-up, and therefore for whom we have most statistical power to detect impacts. It is also the group for whom underage enrollment in primary school is a particularly important part of the counterfactual. Children from the six-year-old cohort were older than 70 months when the 2010/11 school year started—above the official primary school enrollment age. As a result, very few of them were supposed to be attending preschool. Nevertheless, this group includes a small proportion of students who were old enough to have benefited from one year of pre-school and who have spent some time in primary school. Results for this group may reflect the longer impact of the program. Unfortunately for the analysis, that effect is mingled with the direct impact of the primary school rehabilitation. Finally, while the difference in preschool exposure between treatment and control is smaller for the four year-old cohort, results for this group are illustrative, as underage enrollment in primary school is more limited for them.

4.2. RESULTS ON DELAYING ENTRY INTO PRIMARY SCHOOL

Table **4** suggested that the program had unanticipated effects on school participation: the percentage of children who are currently attending in primary school is lower by a statistically significant 10 percentage points in treatment villages. This decline is consistent with a corresponding increase in participation in preschool (25 percentage points). There is no significant effect of the program on ever attending any formal school" (that is, preschool or primary school) suggesting a substitution between preschool and primary school attendance.[19] Compliance by age group is illustrated in Figure 2 where the average *current* attendance rates by age at follow-up for each school type and by treatment status is presented. The significant positive difference in preschool participation induced by the intervention (Panel 2) is offset by a significant negative differential in primary school participation (Panel 1). Overall participation in any school is similar in treatment and control group (Panel 3).

Disaggregating by age suggests that the negative effect on primary school enrollment is largely driven by the five year-old cohort for whom participation in preschool is the highest: for this cohort the increased enrollment in preschool (32 percentage points) is in large part compensated by a decrease in (current) primary school enrollment (by 21 percentage points). While this cohort was not technically eligible for primary school in October 2010 (for school year 2010/11), the share of control group children enrolled in the first grade of primary school is nevertheless very high: almost 60%. Indeed, many children attend primary school before reaching the official minimum age, and informal registration of underage children in grade 1 is common practice.[20] Figure 3 shows the density of the age of first enrollment in primary school by actual treatment status, which clearly shows a shift to the right (older ages) for the treatment group.[21] The mean age of entry increases from 68 months in villages without a preschool to 71 months old in villages with a preschool. We discuss the possible interpretations of this effect in section 6.

4.3. IMPACT OF PROGRAM ON AVERAGE TEST SCORES

[19] Notice that in table 4 primary school attendance is measured "currently" at follow-up while any school and pre-school enrollment are given retrospectively ("ever attended"). To compare attendance, we contrast in figure 2 the probability of "ever attending" primary school, preschool and any school. In figure 4 (see infra) we provide a similar comparison for current enrollment.

[20] Qualitative interviews indicated that minimum age for formal registrations were relatively well enforced: parents have to hand in an official birth certificate or the family book containing birth dates. Nonetheless, a large share of pupils who attended classes were simply not registered, and this group indeed appeared to be younger.

[21] The graph relies on data regarding date of birth and age obtained from caregivers not from the school. When possible, the date of birth was verified using the birth certificates. Nonetheless, parents had no incentive to misreport the date of birth as data collection was unrelated to the enrollment process.

The substitution from primary school to preschool could in theory have a positive or negative effect on children's cognitive and non-cognitive development depending on the quality of teaching and learning in each environment, as well as the age-appropriateness of the approaches used in each setting. Table 6 shows the impact of the preschool program on the cognitive, motor and non-cognitive test scores for the full sample and suggests that, overall, there is very little evidence of any positive impacts of the preschool program. Out of the 13 impacts measured, two are statistically significant (at the 10% level, and only when additional covariates are included), and one of these is negative.[22] The impact is positive for the ASQ gross motor scale; the impact is negative and statistically significant—albeit small—for the ASQ problem solving subscale. Table 7 reports results derived when considering the outcomes grouped by domain. All point estimates for the family outcomes are negative but not statistically significantly different from zero, except maybe for the negative effect on the "Cognitive development index" which is close to the 10 % acceptance level.

Table 8 reports the corresponding results disaggregated by age cohort. The overall impact of the intervention on tests scores for the cohort of five years old is negative—a result driven by the impact on cognitive development, which is large (-0.19 standard deviations) and statistically significant. Impacts for the six year-old cohort, who were neither more likely to enroll in preschool or primary school, are small and statistically insignificant. Yet, as discussed above, results on the 6 years old could also offer a longer-term vision on the impact of the policy: in the treatment group, 22% of the 6 year old children enrolled in primary had benefited from a pre-school program against 11% in the control. While these longer term effect are mingled with other effects via the school rehabilitation[23], absence of effect on the six year olds is suggestive that even after one year, children did not experience positive impacts. Impacts in the four year-old cohort, which can be interpreted as the pure effect of pre-school (since very few of these children have ever registered in primary school), are marginally significant on motor skills, suggesting a possible positive impact for these younger children.

4.4. ROBUSTNESS

Column 1 of Table 9 shows intent-to-treat results on five year olds for the main outcomes without any covariates, the following columns (2 to 4) progressively include additional covariates. In specification (5) we control for the corresponding baseline test score in addition

[22] As expected because of the randomization, point estimates are similar with or without covariates but standard errors decrease when controls are added.
[23] Including better premises, more homogeneous grade 1 classrooms or reduced class-size.

to other covariates. Point estimates remain very similar and significant across all specifications.

Next, we assess the importance of outliers by estimating impacts after removing observations with extreme values of the outcome variable. The three panels of Table 10 exclude observations with values above and below three, two, and one standard deviation, respectively, around the mean. Estimates remain qualitatively similar across the specifications, as well as in comparison to the full sample (compare to column 4 of Table 9).

5. INTERPRETATION AND DISCUSSION

5.1. SUBSTITUTION EFFECT BETWEEN PRIMARY SCHOOL AND PRESCHOOL

Interpreting the negative impacts in the five year-old cohort hinges on a good understanding of the counterfactual. Children from the five year-old cohort in the control group are either at home (where they might get some early education from their parents) or attending primary school through informal underage enrollment. Introducing preschool into this context potentially crowds out those other forms of education. If those induced to attend preschool by the intervention are those who would have been in school (as underage enrollees) in the absence of the intervention, then our negative estimates on the five years old would capture the substitution effect between preschool and primary school (as an underage enrollee). And if those induced to attend preschool would have been at home in the absence of the program, then the estimates would capture the substitution of preschool for home.

At first glance the results in Table 4 may suggest that the former is more likely, as the increase in preschool enrollment is concomitant with a decrease in primary school enrollment. Figure 4 further illustrates that for the five year-old cohort as a whole (first panel), the preschool construction program is associated with a gain of around 20 percentage points in (current) preschool attendance (from 10 percent to 30 percent), and an equivalent decline in (current) primary school attendance—while the share of children not going to school remained constant (around 30% in both groups).

Yet the interpretation depends on whether the children enrolling in preschool are indeed the same children that would enroll in primary school in the absence of the program. If that were the case, the children not enrolled at (any) school should be completely unaffected by the treatment. For them, the school construction's impact would be null as they can be considered "school never takers" and the negative impact found for the five year olds would be fully driven by those who are schooled (and switch between types of schooling). To investigate more in

depth whether substitution is the driving factor, we are reasoning in Table 11 *ad absurdum*, that the treatment had no effect on the follow-up schooling status (preschool or primary school) and hence that the substitution is the only interpretation. Under the assumption that the intervention does not affect children's probability of enrolment in any school, Table 11 would give the unbiased effects of schooled and non-schooled children. The results show, as expected, that the differences in motor and cognitive test scores between treatment and control villages are indeed negative for children who are schooled. Yet, the differences between treatment and control villages are also negative for the children not schooled at follow-up, which is inconsistent with substitution being the sole mechanism. While the latter finding does not rule out that substitution between primary school and preschool may in part explain the negative results on cognition (as negative differences are found for schooled children), it suggests that an additional channel for the negative effects is involved.

5.2. COMPOSITION EFFECT

The negative difference between treatment and control groups among the non-schooled children can be explained by a composition effect, i.e. the children that are enrolled in primary school (in the control group) are not the counterfactual children of the ones enrolled in preschool in the treatment group. Families that choose to send their child to preschool are not necessarily similar to the ones that would have sent the child to primary school (as an underage enrollee) in the absence of the intervention. The middle and right-hand panels of Figure 4 illustrate this process by showing school status at follow up (by group) for children with a literate mother (middle panel) and for those with an illiterate mother (right-hand panel). For children with a literate mother, the probability of being in preschool increases by 31 percentage points (from 7 to 38 percent)—and is associated with a reduction of children who are out of school, and a reduction of children in primary school (as underage enrollees). For children with an illiterate mother, however, the increase in preschool participation of 12 percentage points (from 11 to 24 percent) is associated with only a decrease in the share of children primary school (as underage enrollees), and no decrease in the share that is out of school completely. In fact, there is an increase of 7 percentage point in the share of out of school children (from 29 to 36 percent)—for this group the net effect of the intervention was a decrease in children who were in any formal school environment.

To see whether these patterns hold in a more systematic way, we estimate the following model in which treatment is interacted with baseline parental characteristics:

$$S_{1i} = \beta_0 + \beta_1 T_i + \beta_2 X_{0i} + \beta_3 X_{0i} * T_i + \epsilon_i \quad (3)$$

where S_{1i} is the enrollment in any school at follow-up, X_{0i} some baseline parental characteristics and β_3 measuring their interaction. This analysis is conducted on both the full sample and on the 5 year old's sample. The results (Table 12) point to the finding that children in school at follow-up are disproportionally more from higher socio-economic backgrounds in the treatment than in the control. In both the full sample, and in the sub-group of 5 year olds, treatment effects are larger when households have fewer children, the caregiver are more educated and the mother is taller (and the results are suggestive that this is true for having paper and pen in the house as well as the mother's Raven test score). These results suggest that the preschool construction had the indirect effect of inducing a group of under-privileged families to take their children out of school. Following Angrist, Imbens and Rubin (1996), such group can be considered as school *defiers* in the sense that they exit out of schooling when treated but enroll when not treated.[24]

While the data do not allow us to identify the exact reason for this sorting, it likely resulted in part from a stricter enforcement of the minimum age rules for primary school enrollment in treatment villages. Indeed, in the treatment group, the average primary school registration age is closer to the official age (70 months, see Figure 3). It would appear, therefore, that poorer families who can no longer register their children as underage enrollees in primary school in treatment villages are opting out of any formal school environment: They may lack information about how to register for preschool, they may have low demand for the newly established preschools, or they may not be able to overcome other registration requirements (despite the fact that preschool registration is free).

5.3. HETEROGENEITY AND INEQUALITY

Given that the analysis of preschool participation indicates important differential demand for children based on parental background, we analyze the extent to which overall impacts differ depending on parents' characteristics. Indeed, while ECD interventions are often motivated based on their potential ability to reduce the gap in school readiness between children from poorer and wealthier backgrounds, this may not hold if parents are making the various enrollment tradeoffs discussed above. We carry out this analysis by estimating a model that interacts treatment with parent characteristics:

[24] This only justify our reluctance to present TOT results as they would be biased in presence of defiers (monotonicity assumption violated).

$$Y_{i1} = \beta_1 T_i + \beta_2 P_0 + \beta_3 T_i * P_0 + \beta_4 \boldsymbol{X}_i + \varepsilon_i \quad (4)$$

Where Y_{i1} are cognitive and non cognitive follow-up test score indexes and P_0 is a baseline parental characteristics. The coefficient β_3 captures the differential effect of the program for the subgroup with the specific parental characteristic (over and above the effect of the program overall, and the relationship of the characteristic to the outcome overall).

Results from estimating equations (4) are presented in Table 13 and are consistent with an important role played by the composition effect. Caregiver's literacy strongly interacts with treatment in determining child outcomes (interaction terms are all positive and mostly statistically significant). The same consistency is found for household revenue, whether households have paper and pen in the house, and to a lesser extent to families whose caregiver had low Raven's test score at baseline. For most variables, the negative effect estimated for the lower socio-economic group (β_1) is approximately of the same magnitude than the point estimates for the higher socio-economic group and a test of ($\beta_1 + \beta_3$) suggests that these are never significantly different from zero. This finding suggests that children from educated and wealthier backgrounds who took-up the preschool program did not benefit from the amount of exposure they received (in terms of the child development outcomes measured), while the poorer children, who may have benefited from a school program, were less likely to be in a formal school environment and had hence lower outcomes. Given that enrollment in preschool is influenced by parental characteristics, an alternative pathway for determining the outcomes could be changes in parental involvement at home. However, the parental involvement index is not significantly different in educated or wealthier families, suggesting that educated parents did not compensate for a "bad" preschool. All in all, these results suggest that the intervention did not contribute to closing the gap in early cognitive development. In fact it may have contributed to increasing it for this age cohort of children.

5.4. OTHER POSSIBLE LIMITATIONS OF THE PRESCHOOL INTERVENTION

Beyond the substitution and composition effects discussed above, it is perhaps reasonable to question whether the quality of the preschool services offered may explain the lack of impact on outcomes. Indeed, we do not find that cognitive outcomes are improved as a result of the intervention among the four year-old cohort, for whom underage enrollment in primary school was less widespread. While we have limited quantitative information on quality, there are a number of elements worth considering.

The two school environments share a variety of features. First, by design, the preschools we

evaluate have the same physical infrastructure as the comparison primary schools (since they are co-located). Second, preschool teachers were at least in part recruited from the same pool of applicants as primary school teachers, and their training and supervision were coordinated by the same institutions. Third, both preschool and primary school classes have a similar 3-hour per day session.

But there are important differences as well. First, teacher quality may have been lower in preschool than in grade 1. Preschool teachers were newly recruited: They had less experience, and perhaps lower motivation or skills than their primary school counterparts. Qualitative interviews suggested that preschool teachers were indeed typically younger and had lower wages (possibly because they had less experience) than primary school teachers. Their training, by design, was also different as it was focused on the preschool curriculum for mixed age groups.

Another potentially important factor that differentiates the two settings is the curriculum itself. The curriculum in primary school focuses mostly on schooling competences (writing, reading, calculus). On the other hand, the preschool curriculum is play-based, and activities such as counting and vocabulary are integrated with physical activities, singing, games, or other age-appropriate activities that cater to a mixed age group of children ages 3 to 5 years. While the preschool curriculum seemed developmentally appropriate, it is possible that the curriculum provided in grade 1 was more conducive to cognitive and fine motor gains among five year-old children.[25]

5.5. Difference with other findings from Cambodia

As indicated in the introduction, our findings differ from those reported in Rao et. al (2012) who study seemingly similar preschools in the same Cambodian context. In their analysis they conclude that "something is better than nothing" as their findings point to an effect size of 1.68 (on the Cambodian Development Assessment Test) of State preschools versus a control group. However the approach used to reach this conclusion differs substantially from ours in at least three significant ways: First, Rao et al. (2012) sample students who have attended preschool programs that were established prior to 2000—i.e. that have been in operation for a long time.

[25] One could hypothesize that gains from stimulation in preschool might have been offset by increases in other early childhood risk factors (such as for instance health setbacks due to frequent contacts with other sick children in preschool.) We therefore analyzed impacts on a wide set of intermediary outcomes related to health, nutrition and stimulation and found no results supporting this hypothesis (results available from authors).

These are programs that have therefore had time to mature and potentially offer better quality, and to whom the community has become accustomed. Second, the "treatment" and "control" villages in Rao et al. (2012) are not randomly assigned. As indicated in the paper, treatment villages have self-selected, or were selected by officials, to have a preschool, while control villages were, by construction in the analysis, villages that have not chosen, or were not chosen by officials, to receive a preschool. The villages are likely, therefore, to differ along a number of observed and unobserved dimensions.[26] Third, Rao et al. (2012) compare children who chose to attend a preschool in the preschool villages versus children who chose to not attend any school in the control villages. As our analysis points out, not everyone with access to a preschool actually attends one, and when there is no preschool in a village some children enroll in primary school as underage enrollees.

6. CONCLUSION

Given the importance of early childhood development for outcomes later in life, early childhood interventions are often considered promising interventions with long-term pay-offs. In the context of a developing country, they may also compensate for existing socio-economic gradients in cognitive development, and hence potentially address one of the root causes of existing inequalities. However, relatively little is known on the impact of preschool interventions in low-income settings, particularly regarding large scale interventions. Due to their potential scalability, preschools are often seen as particularly promising for reaching many disadvantaged children at once.

In the context of this study, we find minimal impacts on the overall development of targeted children, which can probably be attributed in part to severe implementation constraints, low take up rates, and a short duration of program exposure for those children who participated. Our evaluation also reveals a surprising negative impact of preschool participation on the cognitive development of the cohort with the highest exposure to the program (the five year-old cohort). This is consistent with two phenomena. First, many five-year old children—mostly from better socioeconomic backgrounds—who attend the newly established preschools would have attended primary school as underage enrollees in their absence. Second, many

[26] While the authors control for the differences in some key variables, such as maternal education and type of livelihood, simply doing so is unlikely to account for all existing important differences. Differences in relevant observed variables are large, suggesting that those for unobserved variables are likely to be large as well.

five-year old children—mostly from worse socioeconomic backgrounds—who would have been enrolled in primary school as underage enrollees in the absence of the program leave the formal school system entirely when the official age of primary school enrollment is enforced. The negative impacts on cognitive development are the largest for these children from less educated and poorer parents, thus resulting in increased inequality.

Due to delays in program implementation, these impacts were all measured shortly after program exposure, and length of exposure was limited. These constitute important caveats to the results. It is possible that a longer exposure time might allow for positive impacts to materialize for the overall group of beneficiary children and/or mitigate some of the negative effects we observe among five-year-olds. That said, we don't find evidence of such a positive long-term effect among the six year olds, for whom at least a small fraction benefited from both preschool and primary school. As preschools become more established, demand among the poor might increase. As preschool teachers gain more experience, it is possible that they could improve the age-appropriateness and effectiveness of their teaching.

Nevertheless, our findings suggest that, at least in the short-term, underage primary school enrollment for children from wealthier and poorer families alike led to more equitable outcomes than the enrollment patterns following the implementation of the preschool program. While higher exposure to preschool and a reduction of early enrollment in primary school might be expected to result in positive impacts, our findings suggest this was not the case in Cambodia, in part because it led to decreased school participation among the most disadvantaged. And while it is possible that the newly constructed preschools might become more effective over time, the cohorts that were exposed in the early years and studied in this paper, suffered early childhood set-backs that might be hard to catch up from. This suggests that the impact of preschool programs can be highly context-specific and determined in large part by the—sometimes unexpected—behavioral responses to an intervention. It also suggests that a better understanding of the behavioral underpinnings, and the more general determinants of the counterfactual, could potentially inform better design of preschool program in Cambodia as well as in other similar contexts.

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Figure 1 : Project timeline

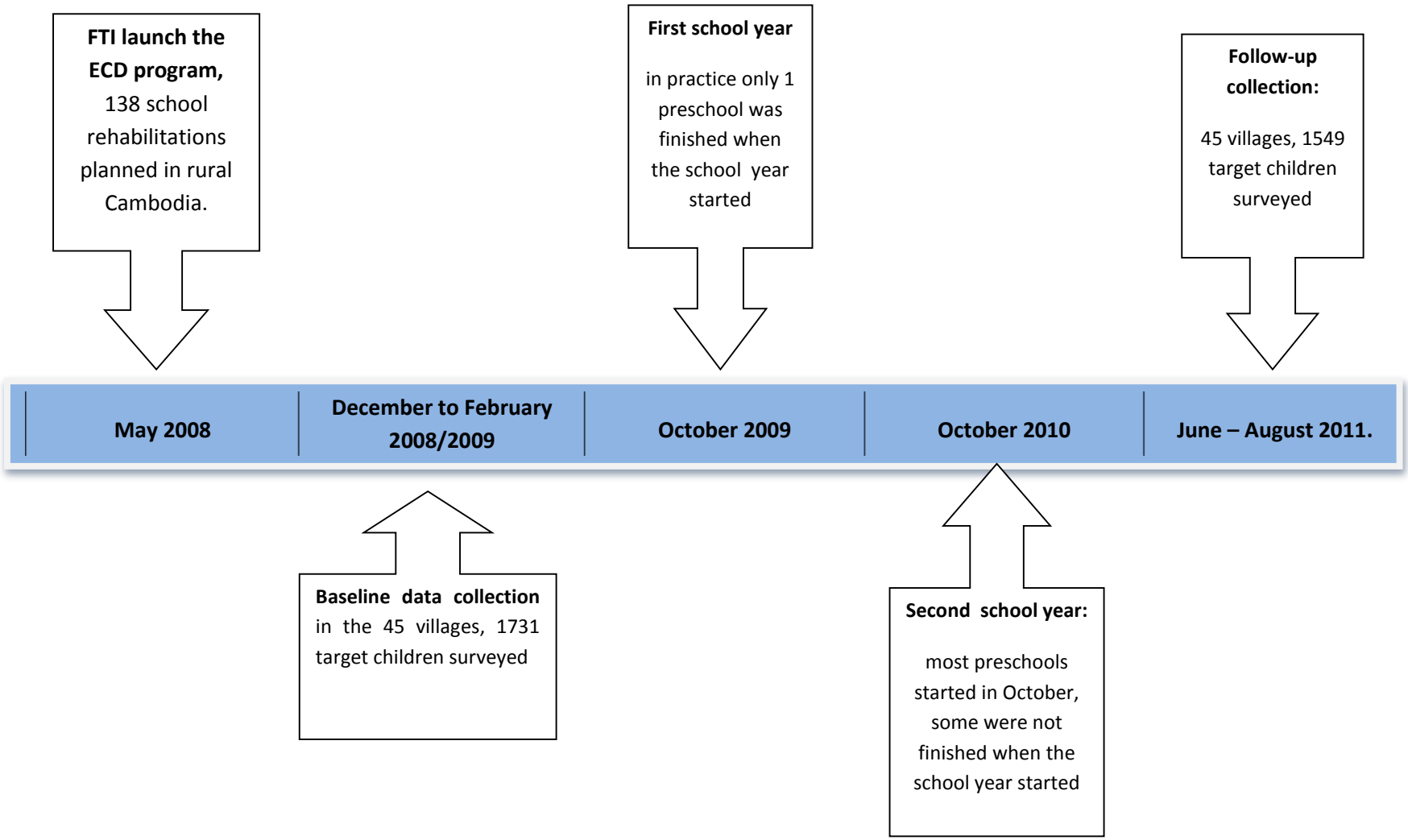

Figure 2: Participation in primary school, preschool or any school, by age

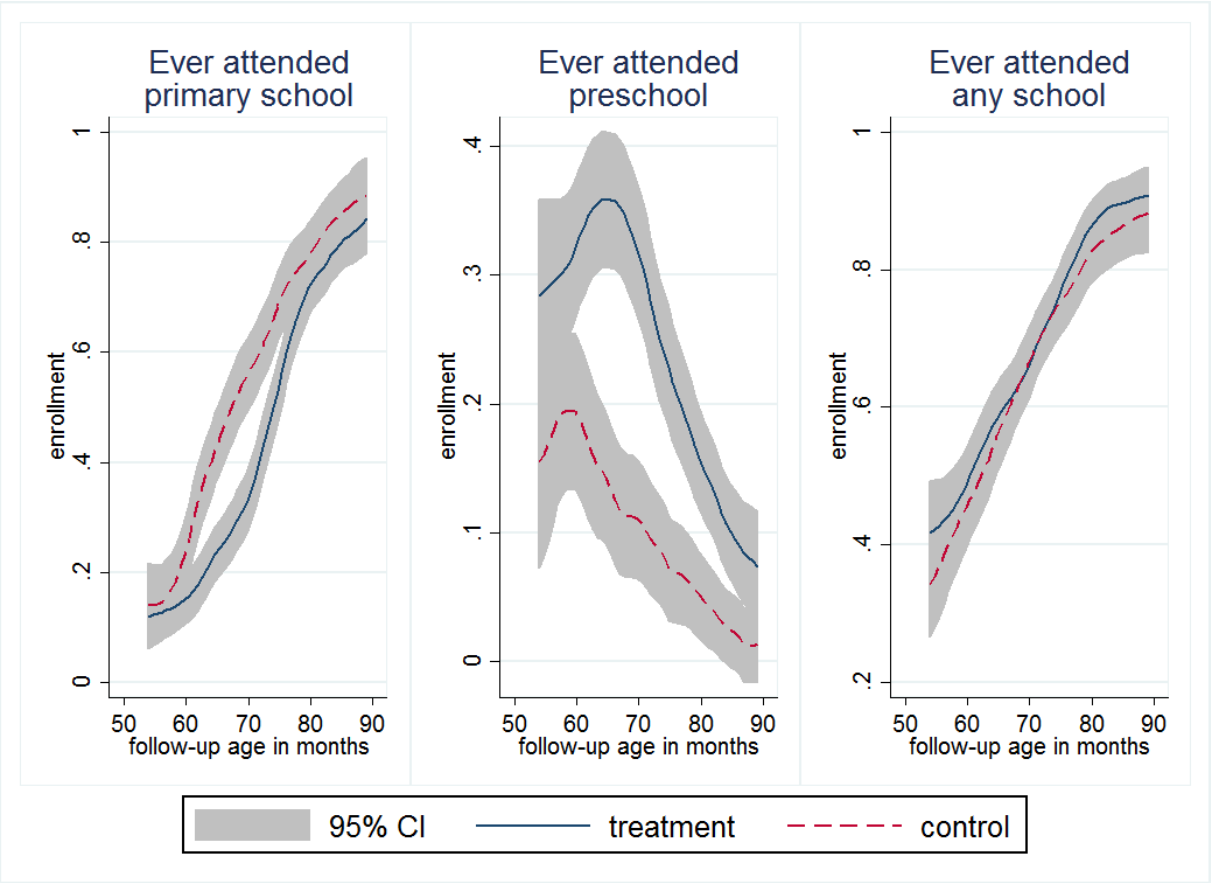

Figure 3 : Primary school enrollment age density by actual village treatment status

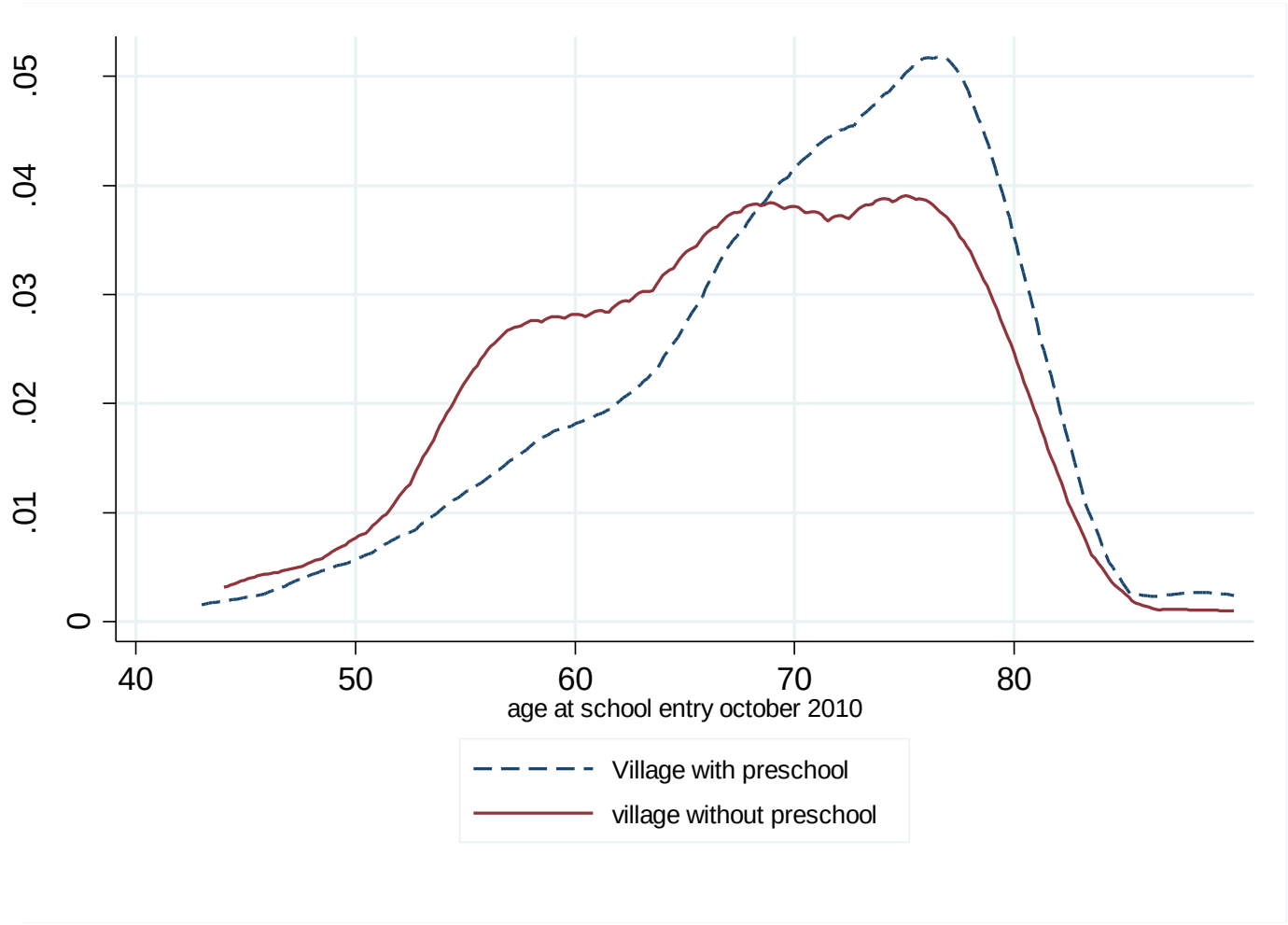

Figure 4: School attendance of 5 year old cohort: composition effect

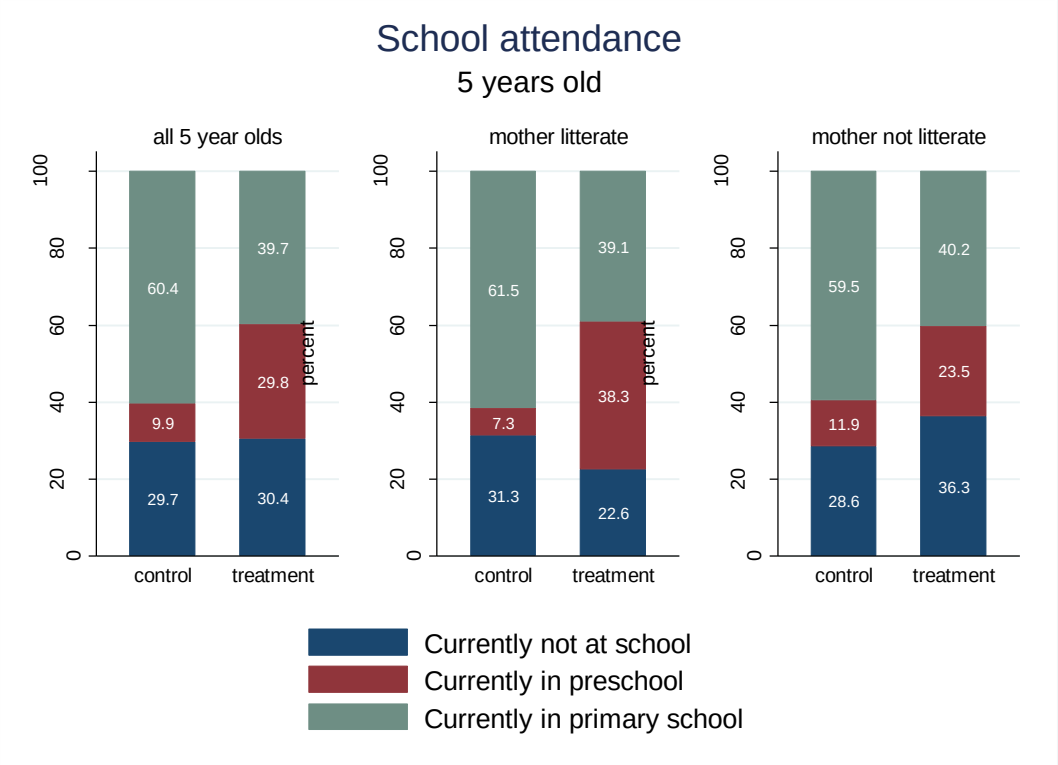

TABLE 1
DESCRIPTIVE STATISTICS AND BALANCE CHECK FOR BASELINE SAMPLE

	Full Sample					5 year old cohort		
	obs.	*average*	*Control*	*Treat-control*	*(s.e.)*	*obs.*	*Treat-control*	*(s.e.)*
Children								
Age in months	1731	41.469	41.259	0.345	(0.576)	599	-0.16	(0.339)
Male	1731	0.525	0.52	0.009	(0.023)	599	0.073*	(0.038)
Attendance at school	1731	0.064	0.063	0.001	(0.025)	599	0.013	(0.020)
Attendance at preschool	1731	0.023	0.031	-0.014	(0.018)	599	-0.003	(0.015)
PPVT score	1176	5.501	5.484	0.028	(0.086)	599	-0.073	(0.096)
ASQ communication	1157	3.101	3.106	-0.009	(0.102)	585	0.079	(0.090)
ASQ gross motor	1159	4.887	4.881	0.01	(0.067)	586	0.048	(0.084)
ASQ fin motor	1157	2.272	2.29	-0.03	(0.068)	584	0.036	(0.089)
ASQ problem solving	1156	2.398	2.422	-0.039	(0.072)	584	0.023	(0.084)
Woodcock-Johnson raw score	1154	1.102	1.06	0.07	(0.084)	582	-0.054	(0.085)
Height-for-age z score	1725	-2.11	-2.093	-0.029	(0.078)	598	-0.087	(0.080)
Household composition								
Number of members in household	1731	5.71	5.623	0.143	(0.153)	599	0.194	(0.160)
Number of children below 6	1731	1.566	1.477	0.146**	(0.064)	599	0.069	(0.074)
Number of adults	1731	2.658	2.704	-0.076	(0.081)	599	-0.112	(0.083)
Economy								
Revenue (per hh memb)	1730	3.928	3.276	1.073	(1.419)	599	0.923	(1.652)
Revenue from paid hours father	1730	6.144	4.47	2.755	(2.431)	599	2.211	(3.167)
Revenue from paid hours mother	1597	1.965	1.406	0.923	(0.846)	545	2.152	(1.457)
Live under a thatch roof	1731	0.354	0.384	-0.051	(0.069)	599	-0.041	(0.072)
Competences/involvment of parents								
Household head years of education	1563	3.055	2.901	0.252	(0.392)	552	-0.029	(0.391)
Caregiver years of education	1621	2.066	1.974	0.153	(0.288)	563	0.327	(0.333)
Household head is literate	1728	0.634	0.623	0.018	(0.051)	599	-0.011	(0.068)
Caregiver is literate	1731	0.417	0.445	-0.046	(0.058)	599	-0.004	(0.067)
Household head attended school	1701	0.747	0.735	0.019	(0.046)	589	0.016	(0.054)
Caregiver attended school	1731	0.653	0.642	0.019	(0.052)	599	0.052	(0.068)
Mother's Raven score	1730	2.346	2.344	0.005	(0.089)	599	-0.006	(0.117)
Parental involvement score	1731	4.616	4.654	-0.062	(0.096)	599	0.003	(0.109)
Have paper & pen at home	1731	0.784	0.775	0.015	(0.032)	599	0.004	(0.037)
Mother's height	1731	153.12	153.23	-0.179	(0.514)	599	-1.07*	(0.637)

Note: Columns *Treat-Control* report the results of the difference between treatment and control. Test scores are standardized. Standard errors are robust and account for intra-village correlation.
* 10% significant level ** 5% significant level *** 1% significant level

TABLE 2
COEFFICIENT ON TREATMENT, ATTRITION AND INTERACTION AT BASELINE

	full sample			5 year olds		
	N	β	δ	N	β	δ
Children						
Age	1731	-0.001	-0.002	599	0.005	-0.005
		(0.001)	(0.001)		(0.005)	(0.006)
Male	1731	-0.017	0.03	599	-0.064*	0.086*
		(0.021)	(0.029)		(0.037)	(0.049)
Summary index Motor	1729	-0.011	-0.011	599	-0.025	-0.06
		(0.012)	(0.016)		(0.042)	(0.067)
Summary index Cognition	1176	0.005	-0.021	599	-0.012	-0.01
		(0.012)	(0.016)		(0.02)	(0.027)
Height for age z score	1731	0.009	-0.03	599	0.034*	-0.072***
		(0.015)	(0.019)		(0.02)	(0.024)
Household composition						
# household members	1731	-0.004	0.003	599	0.008	-0.012
		(0.008)	(0.01)		(0.01)	(0.013)
# children below 6	1731	0.02	-0.005	599	-0.041	0.053
		(0.029)	(0.034)		(0.031)	(0.039)
Economy						
Revenue household head	1731	-0.009	0.009	599	0.013	-0.031
		(0.012)	(0.015)		(0.021)	(0.025)
Live under a thatch roof	1730	0.003*	0.001	599	0.001	0.006**
		(0.002)	(0.003)		(0.002)	(0.003)
Hh head education (year)	1731	-0.007	0.014	599	-0.024	0.045
		(0.029)	(0.041)		(0.025)	(0.048)
Parents						
Hh head education (year)	1563	0.007	0	552	0.011	-0.005
		(0.005)	(0.006)		(0.01)	(0.012)
Caregiver education (year)	1621	-0.003	0.009	563	0.009	-0.008
		(0.006)	(0.008)		(0.013)	(0.014)
Household head literate	1728	0.047**	-0.009	599	0.026	0.018
		(0.023)	(0.033)		(0.042)	(0.057)
Caregiver read literate	1731	-0.027	0.036	599	0.003	0.007
		(0.027)	(0.034)		(0.038)	(0.048)
Raven test score	1730	0.033**	0.006	599	0.033	-0.003
		(0.012)	(0.017)		(0.022)	(0.029)
Have paper & pen at home	1731	-0.072**	0.026	599	0.027	-0.045
		(0.034)	(0.045)		(0.042)	(0.061)
Mothers height	1731	0.004*	-0.008***	599	0.009**	-0.015***
		(0.002)	(0.003)		(0.004)	(0.005)

The above table presents the results from regression (1): listed in the first column are the baseline characteristics. Column N gives the number of observation, column β the effect of the baseline characteristics on the attrition level, and δ the interaction between the baseline characteristics and the treatment variable (the attrition bias). Standard errors are robust and clustered at the village level but not control for any additional baseline variables.

* 10% significant level ** 5% significant level *** 1% significant level.

TABLE 3
PRESCHOOLS AT FOLLOW-UP: VILLAGE LEVEL STATISTICS

	Total	*Control*	*Treatment*
Number of villages	45	19	26
Number of villages with a preschool at follow-up (*admin data*)	21	2	19
Number of villages with a preschool at follow-up (*school survey*)	24	3	21

Notes: The table presents the number of villages in the control and treatment group for different subsample and from different sources of information (administrative data and surveys with school directors and village chiefs).

TABLE 4
PARTICIPATION IN PRESCHOOL AND PRIMARY SCHOOL

		(1) *Obs.*	*(2)* *Control*	*(3)* *Treat.*	*(4)* *Treat.- Control*	*(5)* *(S.E.)*
1	**Ever attend formal *preschool* program**					
a	… in full sample	1548	0.106	0.358	0.252***	(0.053)
b	… on 4 year olds	489	0.119	0.368	0.249***	(0.073)
c	… on 5 year olds	534	0.090	0.410	0.320***	(0.055)
d	… on 6 year olds	525	0.110	0.299	0.189***	(0.062)
e	… in villages with a functioning preschool	949	0.382	0.353	-0.029	(0.125)
2	**Currently in *primary* school**					
a	… in full sample	1547	0.561	0.460	-0.101**	(0.048)
b	… on 4 year olds	489	0.244	0.156	-0.088	(0.07)
c	… on 5 year olds	534	0.604	0.398	-0.206***	(0.071)
d	… on 6 year olds	524	0.832	0.789	-0.043	(0.039)
3	**Ever attend formal *school* system**					
a	… in full sample	1547	0.660	0.698	0.038	(0.046)
b	… on 4 year olds	489	0.409	0.476	0.067	(0.075)
c	… on 5 year olds	534	0.703	0.696	-0.007	(0.064)
d	… on 6 year olds	524	0.864	0.898	0.034	(0.032)
4	**Preschool duration (months)**					
a	… in full sample	1548	0.649	2.642	1.993***	(0.477)
b	… in villages with a functioning preschool	401	6.141	7.383	1.242	(0.921)
5	**Ever attend Community Center-based Program**	1548	0.129	0.055	-0.074	(0.063)
6	**Ever receive a home visit or community meetings**	1548	0.208	0.148	-0.060	(0.043)

Note: column 2 gives the average of the dependent variable in the control group, column 3 the one in the treatment group. Column 4 gives the ITT estimates of the dependent variables. Standard errors in column 5 are robust and account for intra-village correlation.

* 10% significant level ** 5% significant level *** 1% significant level

TABLE 5
CORRELATES OF PRESCHOOL PARTICIPATION AT FOLLOW-UP

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*
Gender (1=male)	0.850*	0.846*	0.826*	0.878	0.850*	0.842*	0.853
	(0.081)	(0.079)	(0.082)	(0.095)	(0.076)	(0.080)	(0.101)
Age	0.984*	0.983**	0.982**	0.983**	0.982**	0.983**	0.983**
	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)
Number of target children in household		0.663***	0.700***	0.645***	0.665***	0.678***	0.678***
		(0.078)	(0.081)	(0.072)	(0.079)	(0.083)	(0.079)
Number of older siblings (>6)		0.923*	0.938	0.920	0.931	0.915*	0.926
		(0.043)	(0.043)	(0.049)	(0.043)	(0.043)	(0.047)
Number of adult (>18)		1.046	1.021	1.022	1.050	1.034	0.998
		(0.070)	(0.065)	(0.075)	(0.071)	(0.070)	(0.067)
Thatch roof (1=yes)			0.580**				0.603**
			(0.138)				(0.134)
Revenue of the household			1.010*				1.007
			(0.006)				(0.006)
Mother literate				2.331***			2.147***
				(0.534)			(0.458)
Mother Raven's score					1.188*		1.063
					(0.107)		(0.089)
Parental involvement score						1.161*	1.047
						(0.105)	(0.111)
Observation	861	861	860	793	860	861	791

Each column presents the results of a logit model and gives the odd ratio for each explanatory variable. Regressions are restricted to the villages with a preschool. Each regression model controls for the treatment assignment. Standard errors are robust and account for intra-village correlation.

* 10% significant level ** 5% significant level *** 1% significant level

TABLE 6
INTENT-TO-TREAT ESTIMATES: FULL SAMPLE

	Obs.	(1) *Coef.* *(S.E.)*	(2) *Coef.* *(S.E.)*
Receptive Vocabulary (PPVT)	1542	-0.026	-0.034
		(0.085)	(0.068)
Ages and Stages Questionnaire			
Communication	1532	-0.105	-0.105
		(0.093)	(0.086)
Gross Motor	1530	0.09	0.101*
		(0.056)	(0.051)
Fine Motor	1531	-0.046	-0.069
		(0.088)	(0.069)
Problem Solving	1530	-0.101	-0.124*
		(0.091)	(0.071)
Memory (Woodcock Johnson)	1533	0.032	-0.015
		(0.083)	(0.064)
Strength and Difficulties Questionnaire			
Emotion	1545	0.04	0.055
		(0.062)	(0.06)
Conduct	1545	-0.064	-0.045
		(0.083)	(0.084)
Hyperactivity	1545	-0.009	-0.018
		(0.08)	(0.075)
Peer	1545	-0.01	-0.001
		(0.072)	(0.07)
Pro-social	1545	-0.086	-0.08
		(0.074)	(0.063)
Anthropometrics			
Height-for-age z-score	1524	-0.02	-0.004
		(0.078)	(0.042)
Weight-for-age z-score	1529	-0.014	-0.011
		(0.055)	(0.044)
Covariates		No	Yes

Note: The table presents *intent-to-treat* estimates of the impact of the program on follow-up cognitive, non-cognitive, motor and anthropometrics measures with different set of covariates. Column 1 shows results without any covariate, column 2's results control for baseline age, gender, age gender interaction dummies, number of children in household, height for age at baseline, and province fixed effects. Standard errors are robust and account for intra-village correlation (45 clusters). All test scores are standardized using the standard error of the control group.
* 10% significant level ** 5% significant level *** 1% significant level

TABLE 7
IMPACT ON FAMILY OF OUTCOMES: FULL SAMPLE

		(1)		(2)	
	Obs.	*Coef.*	*(S.E.)*	*Coef.*	*(S.E.)*
Overall development index	1549	-0.029	(0.048)	-0.031	(0.035)
Cognitive development index	1542	-0.064	(0.075)	-0.082	(0.056)
Motor development index	1532	0.022	(0.06)	0.016	(0.048)
Anthropometrics index	1541	-0.017	(0.063)	-0.008	(0.035)
Non cognitive index	1545	-0.026	(0.051)	-0.018	(0.044)

Note: Results from the Seemingly Unrelated Regression model (SUR). Column 1 shows results from regressions without any covariates, while column 2 shows results with the more complete set of covariates (baseline age, gender, age gender interaction dummies, number of children in household, height for age mother height and province fixed effect). Overall development index accounts for all tests scores; cognitive development index is an index of all cognitive tests (vocabulary, memory, problem solving and communication of the ASQ); Motor development index is composed of gross motor and fine motor; Anthropometrics index includes weight-for-age and height-for-age; and the non-cognitive index corresponds to the index of the subscales of the Strength and Difficulties Questionnaire. Standard error (s.e.) are robust and account for intra-village correlation.

* 10% significant level ** 5% significant level *** 1% significant level

TABLE 8
SUR REGRESSIONS BY COHORT

	4 year old cohort			5 year old cohort			6 year old cohort		
	Obs.	*Coef.*	*(S.E)*	*Obs.*	*Coef.*	*(S.E)*	*Obs.*	*Coef.*	*(S.E)*
Overall development index	487	0.009	(0.045)	533	-0.063*	(0.034)	523	-0.023	(0.051)
Cognitive development index	484	-0.036	(0.072)	530	-0.189***	(0.065)	522	0	(0.091)
Motor development index	481	0.163*	(0.095)	527	-0.089	(0.06)	519	-0.015	(0.043)
Anthropometrics index	485	-0.02	(0.055)	533	-0.023	(0.051)	517	0.032	(0.051)
Non cognitive index	487	-0.006	(0.056)	529	0.032	(0.055)	523	-0.065	(0.071)

Note: results from the Seemingly Unrelated Regression model (SUR). All estimates control for age, gender, age gender interaction dummies, number of children in household, height for age at baseline and province fixed effect. Overall development index accounts for all tests scores; cognitive development index is an index of all cognitive tests (vocabulary, memory, problem solving and communication of the ASQ); Motor development index is composed of gross motor and fine motor; Anthropometrics index includes weight-for-age and height-for-age; and the non-cognitive index corresponds to the subscales of the Strength and Difficulties Questionnaire. Standard errors (s.e.) are robust and account for intra-village correlation.

*10% significant level **5% significant level *** 1% significant level

TABLE 9
ROBUSTNESS OF TREATMENT EFFECT FOR DIFFERENT SPECIFICATIONS: 5 YEAR OLD COHORT

	(1)		(2)		(3)		(4)		(5)	
	Obs.	*Coef. (S.E)*	*Obs.*	*Coef. (S.E)*	*Obs.*	*Coef. (S.E)*	*Obs.*	*Coef. (S.E)*	*Obs.*	*Coef. (S.E)*
Overall development index	534	-0.074	534	-0.072	533	-0.063*	533	-0.063*	533	-0.064*
		(0.047)		(0.046)		(0.038)		(0.034)		(0.033)
Cognitive development index	531	-0.187**	531	-0.203**	530	-0.193***	530	-0.189***	530	-0.191***
		(0.086)		(0.084)		(0.075)		(0.065)		(0.066)
Motor development index	528	-0.107	528	-0.099	527	-0.092	527	-0.089	527	-0.093*
		(0.068)		(0.066)		(0.061)		(0.060)		(0.057)
Anthropometrics index	534	-0.045	534	-0.042	533	-0.025	533	-0.023	533	-0.022
		(0.075)		(0.075)		(0.052)		(0.051)		(0.050)
Non cognitive index	530	0.018	530	0.032	529	0.037	529	0.032	529	0.032
		(0.054)		(0.054)		(0.055)		(0.055)		(0.055)
Controls:										
Age, gender and interactions				✓		✓		✓		✓
Child baseline height-for-age and mother height						✓		✓		✓
Region fixed effect and household composition								✓		✓
Baseline test score										✓

Note: results from the Seemingly Unrelated Regression model (SUR) on five year old for different set of covariates. Overall development index accounts for all tests scores; cognitive development index is an index of all cognitive tests (vocabulary, memory, problem solving and communication of the ASQ); Motor development index is composed of gross motor and fine motor; Anthropometrics index includes weight-for-age and height-for-age; and the non-cognitive index corresponds to the subscales of the Strength and Difficulties Questionnaire. Standard errors (s.e.) are below in parenthesis. They are robust and account for intra-village correlation.
* 10% significant level ** 5% significant level *** 1% significant level

TABLE 10
INTENT TO TREAT RESULTS EXCLUDING OUTLIERS: 5 YEAR OLD COHORT

	Truncation above +/- 3 sd			Truncation above +/- 2 sd			Truncation above +/- 1 sd		
	Obs.	*Coef.*	*(S.E)*	*Obs.*	*Coef.*	*(S.E)*	*Obs.*	*Coef.*	*(S.E)*
Overall development index	529	-0.062*	-0.034	526	-0.069**	-0.032	434	-0.022	-0.027
Cognitive development index	525	-0.186***	-0.064	518	-0.182***	-0.064	410	-0.124***	-0.045
Motor development index	523	-0.103*	-0.057	516	-0.1*	-0.056	415	-0.032	-0.048
Anthropometrics index	530	-0.027	-0.05	513	-0.033	-0.047	381	0.028	-0.036
Non cognitive index	527	0.031	-0.053	505	0.021	-0.047	373	0.003	-0.032

Note: Table presents the ITT results for the 5 year old cohort after excluding selected outliers. Regressions include usual controls and can be compared to results in column 4 of Table 9.

TABLE 11
DIFFERENCES BETWEEN TREATMENT AND CONTROL AT FOLLOW-UP BY FOLLOW-UP SCHOOLING STATUS: 5 YEAR OLD COHORT

	Schooled			***Not schooled***			***Difference between Schooled and Non Schooled***	
	Obs.	*Coef.*	*(S.E)*	*Obs.*	*Coef.*	*(S.E)*	Coef.	*P-value*
Overall development index	372	-0.058	(0.039)	161	-0.09	(0.056)	0.032	0.637
Cognitive development index	371	-0.156**	(0.068)	159	-0.273***	(0.102)	0.118	0.339
Motor development index	370	-0.108*	(0.062)	157	-0.099	(0.169)	-0.009	0.959
Anthropometrics index	372	-0.068	(0.057)	161	0.069	(0.075)	-0.138	0.146
Non cognitive index	371	0.045	(0.07)	158	-0.003	(0.061)	0.049	0.612

Note: Table presents the results for the estimation of equation 2 for the 5 year old cohort, estimated separately by follow-up schooling status (schooled or not schooled at follow-up) for the aggregate indexes (see note table 7). The last columns give the difference between the coefficient for schooled and not schooled and its significance level. Standard errors (s.e.) are robust and account for intra-village correlation.

* 10% significant level ** 5% significant level *** 1% significant level

TABLE 12
HETEROGENEOUS EFFECTS BY BASELINE PARENTAL CHARACTERISTICS ON FOLLOW-UP SCHOOLING

	Full Sample		5 year olds	
	Interaction coefficient	(S.E.)	Interaction coefficient	(S.E.)
Number of children below 6 in hh	-0.09*	(0.046)	-0.152***	(0.055)
Revenue (per hh member)	0.003	(0.002)	0.003	(0.003)
Live under a thatch roof	-0.039	(0.047)	-0.036	(0.065)
Household years of education	-0.005	(0.008)	0.008	(0.012)
Caregiver years of education	0.006	(0.011)	0.017	(0.019)
Household head is literate	0.033	(0.056)	0.1	(0.088)
Caregiver read is literate	0.106*	(0.057)	0.193**	(0.087)
Head attend formal school	-0.006	(0.059)	0.037	(0.079)
Caregiver attend school	0	(0.046)	0.002	(0.074)
Raven score	0.052*	(0.03)	0.069	(0.046)
Parental involvement score	0.008	(0.027)	0.025	(0.039)
Have paper & pen at home	0.065	(0.065)	0.148	(0.107)
Mother height	0.007**	(0.003)	0.01*	(0.006)

We present the result of the interaction model (3). Only the interaction coefficients are displayed with its robust and cluster at village level standard error. We present the result for the full sample and for the sample of the 5 year olds. We control by the baseline children's age, sex cognitive index, motor skills index, height for age, initial school enrollment as well as for the province fixed effects.
* 10% significant level ** 5% significant level *** 1% significant level

TABLE 13
HETEROGENEOUS TREATMENT EFFECT: 5 YEAR OLD COHORT

	Overall development index		Motor development index		Cognitive development index		Parental involvment index	
	Coef.	*(S.E)*	*Coef.*	*(S.E)*	*Coef.*	*(S.E)*	*Coef.*	*(S.E)*
Characteristic 1								
Caregiver literate	-0.031	(0.113)	-0.031	(0.110)	-0.07	(0.115)	0.257*	(0.133)
Treatment	-0.164**	(0.068)	-0.176**	(0.081)	-0.314***	(0.087)	-0.06	(0.158)
Caregiver literate*Treatment	0.261*	(0.132)	0.232*	(0.134)	0.353**	(0.144)	-0.002	(0.204)
Characteristic 2								
Household revenue	0.001	(0.003)	-0.009	(0.006)	-0.005	(0.004)	-0.004	(0.005)
Treatment	-0.068	(0.063)	-0.115	(0.070)	-0.171**	(0.070)	-0.137	(0.113)
Hh revenue *Treatment	0.006	(0.004)	0.016**	(0.007)	0.015**	(0.006)	0.002	(0.009)
Characteristic 3								
Raven score	-0.032	(0.080)	0.062*	(0.032)	0.08	(0.059)	0.134	(0.088)
Treatment	-0.305*	(0.152)	-0.108	(0.142)	-0.317*	(0.176)	-0.056	(0.292)
Raven score *Treatment	0.109	(0.081)	0.015	(0.053)	0.08	(0.068)	-0.033	(0.105)
Characteristic 4								
Parental involvement	-0.059	(0.072)	0.052	(0.044)	-0.018	(0.033)	0.288***	(0.060)
Treatment	-0.659**	(0.326)	0.16	(0.292)	-0.492**	(0.238)	-0.048	(0.435)
Involvement *Treatment	0.132*	(0.075)	-0.05	(0.064)	0.078*	(0.046)	-0.017	(0.090)
Characteristic 5								
Paper & pen	-0.074	(0.056)	-0.01	(0.124)	-0.21**	(0.093)	0.501*	(0.259)
Treatment	-0.253***	(0.063)	-0.127	(0.128)	-0.479***	(0.134)	-0.13	(0.272)
Paper & pen *Treatment	0.252***	(0.080)	0.068	(0.147)	0.438***	(0.133)	-0.019	(0.292)

The table gives the results of the interaction term of regression equation (4) for five different parental baseline characteristics: caregiver is literate, household revenue, raven score, an index of parental involvement and whether the household has paper and pen.
* 10% significant level ** 5% significant level *** 1% significant level

ANNEX TABLE A
EXPOSURE TO SCHOOL: CHILD-LEVEL STATISTICS

	(1)	(2)	(3)	(4)	(5)
	Obs.	*Control*	*Treatment*	*T-C*	*(S.E.)*
Village has a primary school according to survey	1731	1	1	0	.
Village has a formal preschool according to survey	1731	0.131	0.817	0.686***	(0.125)
Village has a formal preschool according to admin	1731	0.128	0.733	0.605***	(0.121)
Preschool classes were given according to survey	1731	0.09	0.773	0.683***	(0.105)
Village has a informal preschool according to survey	1731	0.163	0.044	-0.12	(0.1)
Village has a home based program according to survey	1731	0.131	0.195	0.064	(0.119)

Note: The table uses the administrative data, data from surveys with school directors and village chiefs and combines them with the number children in the targeted cohorts for each village. Column 1 gives the total number of children, column 2 the average children's participation rate in the control group, column 3 the same ratio for the treatment group; column 4 shows the difference between treatment and control and column 5 the standard errors of the difference, robust and clustered at the village level.
* 10% significant level ** 5% significant level *** 1% significant level*